

## Social Science: City Centre or Leafy Suburb<sup>1</sup>

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### Abstract

This article argues, in opposition to a common interpretation of Wittgenstein deriving from Winch, that there is nothing especially problematic about the social sciences. Familiar Wittgensteinian theses about language, notably on the open-endedness of linguistic rules and on the importance of family resemblance concepts, do have great relevance to the social sciences, but also to much of the natural sciences. The differences between scientific and ordinary language are much less sharp than Winch, and probably Wittgenstein, supposed.

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<sup>1</sup> I am honoured to have been invited to speak at the annual conference of the British Wittgenstein Conference, for which the first version of this paper was prepared, and I would like to thank the organisers, Nigel Pleasants and Daniele Moyal-Sharrock, for the invitation. I cannot claim to be a Wittgenstein scholar. However, having started my philosophical life as an undergraduate at St John's College Oxford, under the tutelage of Peter Hacker and the late Gordon Baker, all my early training was coloured with Wittgenstein's thought. I have no doubt that Wittgenstein, or at least an interpretation of Wittgenstein's later philosophy, has shaped much of the work I have done in the philosophy of science.

## Keywords

Ludwig Wittgenstein, Peter Winch, social science, biology, linguistic rule

## **1. Introduction**

My title, as will not have escaped readers of Wittgenstein, alludes to a memorable figure from the *Philosophical Investigations*. Let me begin by quoting this in full:

...ask yourself whether [our language] was [complete] before the symbolism of chemistry and the notation of the infinitesimal calculus were incorporated in it; for these are, so to speak, suburbs of our language...Our language can be seen as an ancient city: a maze of little streets and squares, of old and new houses, and of houses with additions from various periods; and this surrounded by a multitude of new boroughs with straight regular streets and uniform houses.

(Wittgenstein, 1953, §18)

This charming metaphor suggests some kind of radical disjunction between so-called “ordinary language” and the language of science, and while such a disjunction may seem plausible enough when science is represented by these particular examples, in general it is, I think, a serious mistake.

I believe it is also a dangerous mistake, and the main burden of this paper will be to argue that it is a mistake that can lead to serious misunderstanding of

the nature of the social sciences. The kinds of examples Wittgenstein cites here, chemical notation and mathematics, fit easily with a certain view of scientific method. Here is Wittgenstein in the *Blue Book*:

Our craving for generality has another main source: our preoccupation with the method of science. I mean the method of reducing the explanation of natural phenomena to the smallest possible number of primitive natural laws; and, in mathematics, of unifying the treatment of different topics by using a generalization. Philosophers constantly see the method of science before their eyes, and are irresistibly tempted to ask and answer questions in the way that science does. This tendency is the real source of metaphysics, and leads the philosopher into complete darkness. (Wittgenstein, 1958, p. 18)

I am in complete agreement with the general tendency of these remarks. But I have one major disagreement. What Wittgenstein describes is *not* the method of science. It may or may not be the method of physics, but it is certainly not the method of biology, the science I know best, and it certainly should not be the aspiration of social science. What these benighted philosophers do, in fact, is worse than what Wittgenstein suggests: they attempt to do philosophy in the way that they falsely imagine that science is done. Darkness is unsurprising.

I must now crave the reader's indulgence for some digression on a science that surely belongs among the natural sciences, biology. My point will be to

give a little more substantial sense of how this area of science differs from that suggested by the passage just cited from the *Blue Book*. Biology, I want to suggest, is actually a maze of little streets and squares just as intricate as ordinary language. No doubt there are differences, as there might be, for instance, between Cambridge and Vienna. But there are few regular streets or uniform houses. A fortiori, there are unlikely to be any such orderly suburbs in the social sciences.

Having offered in brief outline some reasons for preferring a very different vision of biology, I shall return to the social sciences and suggest that there is nothing so exceptional or anomalous about the scientific study of society. Finally, I shall return to the question of the relation between the languages of science and everyday life. I shall argue that these are not so different as most philosophers, including many who have grounded their views in their reading of Wittgenstein, have supposed. Indeed, the lessons that we have learned from Wittgenstein are deeply relevant to the languages of science, both natural and social. Or so I shall argue.

## **2. Biology**

Back in the 1970s, when logical empiricism was still alive, if in decline, there were quixotic attempts to formalise the theory of evolution (e.g. Williams 1970). These attempts were not successful, however. And in hindsight it is easy enough to see many reasons why success was not to be expected. One such reason should be no surprise to this audience. The formalisation project

assumes that central general terms will have exact definitions, necessary and sufficient conditions for their application. But language doesn't generally work like this. Wittgenstein has taught us to look at the uses of words, and when we do this we do not find any such simple and unambiguous rules for their uses. Nonetheless, many philosophers, and perhaps even Wittgenstein himself, have supposed that science was different. This is one natural way of understanding the urban planning metaphor I cited earlier.

My personal pathway into Wittgensteinian philosophy of biology began with reflections on our general terms for kinds of organisms. When I started doing philosophy in the 1970s, the hottest topic was the so-called new theory of reference, centred on Saul Kripke's ideas about direct reference, unmediated by anything like a Fregean sense. Although these ideas were generally associated with the reference of proper names, Hilary Putnam, and to some extent Kripke himself, proposed extending something similar to general terms, proposing their direct reference to natural kinds.

Putnam wrote, for example, that "the predominant sense of 'lemon' is one in which to be a lemon something has to have the genetic code of a lemon" (1975, p. 240). I should stress that Putnam is very careful not to say that this criterion provides a definition of lemon. Our use of the word continues to be guided by loose pre-scientific criteria—lemons are yellow, sour, etc. But these everyday criteria do not determine the reference of the word. The idea is that science discovers the essences of the kinds that our ordinary language more or less successfully gropes for. Part of the use of our ordinary word

“lemon” is to refer, somehow or other, to the real kinds with real essences described by science.

There are two reasons why this cannot be right. First, much of our ordinary language isn’t even close enough to a candidate “scientific” term to aim for a scientific reference. As I have argued before in some detail, many ordinary language terms serve important and identifiable purposes, but purposes far removed from those of professional taxonomists (Dupré 1981). We are interested in organisms that can be eaten, can be used to build houses or make clothes, and so on, and the extensions of terms in our languages can be traced to their utility for serving these functions.

But second, and even more importantly, science doesn’t provide the kinds of unique and essential definitions that Putnam’s story assumes. Examples of this second point are legion once one starts to look for them. Indeed, the scientific use of the term “species” perfectly illustrates the point<sup>2</sup>. But here I shall turn to an even more iconic term in contemporary biology, one also implicated in Putnam’s example of the lemon, the gene. When we look at the actual use of this word in science we find neither necessary nor sufficient conditions of application, nor even a serious aspiration to decide on such.

The term “gene” was invented by the Danish botanist Wilhelm Johanssen in 1909. The entity to which it referred is often taken to have been discovered by the Moravian monk, Gregor Mendel in the 1860s, whose work was

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<sup>2</sup> See Dupré (1981; 1999); Ereshefsky (1992).

famously ignored until the beginning of the 20<sup>th</sup> century. It became established at the heart of biology through the work of Thomas Hunt Morgan and his collaborators, at Columbia and Cal Tech in the USA, on the fruit fly *Drosophila Melanogaster*. This hyper-Whiggishly summarises a set of stories to which many historians have devoted their lives<sup>3</sup>.

The phenomenon the discovery of which was attributed to Mendel is particulate inheritance. Mendel recorded particular variable features of plants in parents and their offspring across multiple generations, and found results consistent with the hypothesis that organisms received factors from each parent capable of determining the presence of these features. In case the organisms received factors determining different features, one or other was found to be “dominant”, sufficient, that is to say, to override the influence of its rival. These factors were, more or less, what Johannsen almost half a century later would call genes, and which were studied in the quite different context of fruit flies by Morgan.

Mendelian genetics continues to this day, especially in agricultural breeding programmes and in medicine. But it has been largely superseded by a quite different programme of genetics, which originated from Francis Crick, James Watson and others’ iconic exposition of the structure of DNA, the material that mediates central aspects of biological inheritance. Crick and Watson’s breakthrough was soon followed by the unravelling of a pathway by which

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<sup>3</sup> For an excellent more detailed account of this history, see Rheinberger et al. (2015).

DNA could act on the development and functioning of the organism, by providing the template for the production of protein molecules which, in turn, were understood as being the basic functional molecules that underlay the structure and behaviour of the organism. The word “gene” then came to be understood as referring to a portion of DNA sequence that provided the information, or “coded”, for a protein.

As philosophers soon noted, this molecular concept of the gene was quite distinct from, and even incommensurable with, the Mendelian concept<sup>4</sup>. For a start, the Mendelian gene had no application at all except to variable features of the organism: it explained differences. There are no human Mendelian genes for having a head, as everyone has one. But there are lots of proteins in a head. In short, a Mendelian gene is, from a molecular point of view, a variation in the DNA sequence, whereas a molecular gene is a (sufficiently) stable chunk of the sequence, of a particular kind.

But this is far from the end of the story. It soon turned out that only a very small percentage of the genome consisted of protein-coding sequence. At first this led to the suggestion that the remainder was “junk”, useless remains of past evolutionary processes. But subsequently much of this junk has found a use, in various levels of regulation of the cell, including the expression of protein-coding genes. Moreover, much of this “junk” turns out to be buried within what were once taken to be protein-coding genes. The latter, it emerged, consisted of alternating segments of coding and non-coding

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<sup>4</sup> A classic exposition of this point can be found in Hull (1974).



sequence, exons and introns. This fragmentation of coding genes made possible the construction of many different proteins—sometimes thousands—by rearrangement or deletion of some of the sections. And within the non-coding sections could often be found distinct functional elements. Which of these are genes? How much of the discontinuously coding gene is the same gene? And so on. “Gene”, it is now often concluded, refers to any bit of sequence to which a competent geneticist has reason to refer.

With all these advances in molecular genetics, Mendelian genetics has not been abandoned. It still remains a central concern, for example, for plant breeders and medical epidemiologists. But note, finally, that a Mendelian gene need not refer to any specific sequence. Typically a Mendelian gene is a defect in the genome that prevents the proper development of a trait in the organism. But many different sequence errors can cause the same defect. Some 1500 different sequence anomalies, for instance, have been found that lead to cystic fibrosis, a disease caused by the failure to produce a particular functional protein.

In summary, then, the language of biological science does not lead us to an orderly array of systematically related references. It is not that scientific terms do not refer. But the uses to which they are put dictate a motley crew of references: structurally diverse, overlapping and cross-cutting, and so on. In part this merely reflects the complexity and diversity of the phenomena to which this language has been designed to refer. Returning to our framing metaphor, we have, indeed, not only a maze of little streets and squares in

our language but also, underlying this language like the remains of an even more ancient city, the little streets and squares of biological reality.

It is hard not to be reminded of Wittgenstein's discussions on rule-following.

The rules for applying the word "gene" are constantly evolving and as important new phenomena come to light decisions must be made, explicitly or more likely implicitly, as to whether and how the word is to be extended in the light of these phenomena. Science is not the discovery of esoteric objects and their crystalline essences, determining their reference in any imaginable context. In biology, at least, it is rather the construction of various perspectives on an immensely complex domain of ever-changing and deeply intertwined processes<sup>5</sup>.

### **3. Social Science**

I won't apologise for spending so much time on biology, because the understanding of social science is so often derailed by a false contrast with a quite misguided image of the so-called natural sciences. In fact, as I now want to argue, the complexity and diversity of both language and phenomena provide no more objection in principle to a successful social science than they

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<sup>5</sup> This sentence summarizes a great deal. For detailed defence of the need for multiple perspectives on biological phenomena, see Dupré (1993); for the case that these phenomena must be understood as intertwined processes (rather than distinct things, or substances) see Dupré (2012).

do to biological science. We need only avoid unrealistic and unattainable aspirations as to what any science can be expected to achieve.

What might be different about social science? Obviously there is a different subject matter. Winch's (1958) classic critique of social science takes science to be a matter of little more than inductive generalisation and reasonably enough claims, for various reasons, that this will not take us very far in studying society. Not very far; but not nowhere either. Economists, though their more ambitious theories are often subject to well-deserved criticism, can at least provide empirical information about the relationship between, for example, government expenditure and employment, which are sometimes fairly successfully projectable into the future. The limits of such science may be due to many things. It is plausible that other variables will be relevant, and including further variables will produce more successful models. Perhaps some of these variables are unknown, or are difficult to measure. Very importantly, the phenomena may be substantially stochastic, and in principle not very predictable. This might be due in part to the impact of unpredictable events—meteorological, political, etc. Everything I have just said could be said equally, *mutatis mutandis*, of a science such as population ecology, which aims to track the numbers in populations of interacting organisms. There are many variables the nature and importance of which is not fully known, many unpredictable shocks, and so on. As one other example from social science, the rather robust evidence that happiness in a society is an inverse function of socio-economic inequality is an empirical correlation that could well be given a lot more attention.

I think that a lot of variably useful social science is like this; indeed this is probably where Millian science has lasted best. But it does also have important and well-known limits. Most obviously it gives us no understanding. Why do many people feel unhappy in very unequal societies. Envy? A sense of worthlessness by comparison to the more fortunate? Perhaps people are happiest when they feel a wide social solidarity (“we are all in it together” as David Cameron has surprisingly pointed out). Supposing we answer these questions there is another set of questions about why humans are, or come to be, such that they exhibit these responses. But parallel questions can, of course be raised in similar natural sciences, and can sometimes be answered. The question before us is whether there is anything peculiar to the social sciences that makes a parallel scientific pursuit of the relevant questions impossible. And one problem that immediately arises is that it is most doubtful whether there is *anything* very general to be said about how sciences provide the relevant kinds of deeper understanding.

On answer does suggest itself as a quite general way in which deeper understanding of social facts, scientific or otherwise, should be provided. Wittgenstein emphasised the centrality of rules in the structure of social life, and thus the idea that there is something normative about the social. This is an idea that plays a central part in Winch’s arguments against a social science following the methods of the natural sciences. It also raises a hugely complex debate that has carried on at least since Davidson’s famous, or notorious, claim that reasons are just causes. One reason for rejecting this

proposal is that reasons are, unquestionably, normative. If I have a compelling reason for doing something, then *ceteris paribus* I ought to do it. And surely there is nothing normative about causality. I don't propose to address this issue head on. What I do want to suggest is that both Davidsonians on the one hand, and Wittgensteinians (and others) who strongly reject this assimilation on the other, often carry out their arguments as if the question what a cause is were a relatively straightforward one. It is not.

Philosophers who debate the nature of causation offer a range of views. There are still many defenders of the Humean view that causation is ultimately no more than correlation; others are attempting to reintroduce neo-Aristotelian conceptions of causal power, and their ilk; others again prefer to treat causation in a more agent-centred view as grounded in the idea of intervention; and so on. My own view, which strikes me as a natural one for a philosopher strongly influenced by Wittgenstein, is pluralistic. All of these analyses have their roles in our use of causal talk; none of them suffices to provide an analysis of causation in general. The universe manages very well without any uniform kind of cement.

One helpful way of thinking about causal talk generally is as the external analogue of explanation. Causal relations are the facts, or some of the facts, that make our explanations true, or useful. But it is fairly obvious that there is no one kind of explanation. Scepticism about social science was understandable when scientific explanation was widely understood in terms of

the deductive-nomological model, and its correlate in the world was taken to be either Humean universal correlations, or perhaps more modally robust laws of nature. But these ideas are well past their sell-by dates. In the biology that I study no one is much interested in universal correlations still less laws of nature. Regularity is local, specific, and—a point I would be happy to elaborate on at length in another context—always maintained with a great deal of effort.

Given this perspective on the nature of causes, the question whether reasons are a kind of cause seems much less portentous. That people have reasons of various sorts is uncontroversially a kind of fact that is essential to our explanation of what they do. Is it, though, a kind of fact that includes some kind of barrier to elaboration into the kind of explanation that should be called scientific?

Is there, then, anything in principle unscientific about the delineation of the rules that exist in a particular society? I cannot see why. Language is profoundly normative, but this doesn't make the science of linguistics impossible. It is a fact, in English, that plural nouns take plural verb forms. If I want to inform you that cows are herbivorous, I ought to say "cows are herbivorous" and I ought not to say "cows is herbivorous". And that's a fact. Not a very high-level fact, or a very important one, no doubt. As more descriptive linguists have insisted, correctly in my view, there may well be dialects in which "cows is herbivorous" is correct, and it may come to pass that "cows is herbivorous" becomes generally correct English. But not yet.

Are these *scientific* facts? I really don't understand this question. As Austin might have said, the word that wears the trousers is "unscientific". There are lots of ways of being unscientific. "Unscientific" is a term of criticism. Being unscientific is liable to lead one to conclusions that are likely to be false. And the sentence "In English one should not say 'cows is herbivorous'" is true. Might one rather say that this is non-scientific? The only sense I can make of this is that the statement in question is not one that should or could play any part in a science of linguistics. And I simply have no idea what basis there might be for such a claim.

I suppose someone might worry that normative linguistics is a science the claims of which are made true by us rather than by nature (as if, somehow we were not part of nature). But surely this is too quick and stipulative to serve as an argument against the legitimacy of linguistics or, for example, social anthropology as sciences. Another related worry that might possibly serve as an objection to the scientific status of such disciplines is that, as I briefly mentioned a moment ago, linguistic generalisations are highly mutable. Such fine words as *vape*, *normcore*, and *slacktivism*, didn't exist ten years ago, but are now enshrined in the Oxford dictionary. Language is a dynamic entity and truths about it change over time. But if the impermanence of social scientific truths makes them ineligible for science, then we shall also have to dispose of biology. Evolution assures us that the biological entities that exist today are very different from those that existed in the distant past. And this is true at all levels, from biological molecules such as proteins and nucleotides, to ecological systems.

My argument so far is that there is nothing in the rule-governed nature of social facts that presents any particular obstacle to their scientific investigation. To take the argument a little further I need a quick exegetical interpolation. Wittgenstein's implications for the social sciences have very largely been channelled through the well-known account of Peter Winch. According to Nigel Pleasants, in his book *Wittgenstein and the Idea of a Critical Social Theory* (1999), Winch was very substantially mistaken in his interpretation of Wittgenstein. Indeed, according to Pleasants, Winch offers us a Kantian account of social reality of exactly the kind Wittgenstein inveighed against. If this is right, I shall now be considering the ideas of Winch rather than Wittgenstein. (We might perhaps call him Winchenstein, a fitting colleague for the even better-known student of Wittgenstein's rules, Kripkenstein.)

Winch, at any rate, argued that a crucial difference between the social and the natural sciences is that in the former rules come in twice. The natural scientist must learn the rules that concern the practice of her science. She must learn, for example, when it is appropriate to say, "there is an electron" or "that is a kinase". More generally, scientists need to know when two situations are to be counted as belonging to the same kinds. But, according to Winch, the social scientist must also have some kind of knowledge of the rules that govern the practice she is investigating. Thus he writes:

If the judgments of identity—and hence the generalisations—of the sociologist of religion rest on criteria taken from religion, then his



relation to religion cannot be just that of the observer to the observed.

It must rather be analogous to the participation of the natural scientist with his fellow workers in the activities of scientific investigation. ... [A] historian or sociologist of religion must have some religious feeling if he is to make sense of the religious movement he is studying.

The sociologist of religion, it appears, will face the difficult task of engaging simultaneously in two rule-governed social worlds, the world of sociology and the world of religion.

I have to confess I find this strange. Of course if the sociologist is investigating a system of rules then he is engaged with two systems of rules: the rules of sociological discourse and the rules he is investigating. But why must he *participate* in the latter? I mean no disrespect to the tradition of cultural anthropology that does pursue a certain kind of participation with the societies it aims to investigate, and there may be a particular kind of knowledge that requires this kind of methodology. But surely it is not the only kind of knowledge possible of an unfamiliar culture? As an atheist I can perfectly well understand why everyone in a village goes into a large building on Sunday morning and recites various narratives together about what I take to be an imaginary being. I might sympathise less with this practice than could a religious person, but I can understand it fairly well. And even if participatory anthropology gains a certain depth of understanding that is not available to other methods of study, it surely pays a price for this in breadth, or generality?

A more plausible point here is a relatively familiar holistic thought about interpretation. If I explain the villagers' church attendance by noting that they believe that omitting to go to church on Sunday's is a mortal sin, I won't understand this unless I find out what sin is, the special gravity and implications of mortal sin, and perhaps many further beliefs about grace, the love of God, and so on. I mention this rather familiar point in part to reiterate the central thesis of this talk, that social science is not that different from much in the natural sciences. In biology, at least, a similar kind of holism applies equally. In both cases it can provide severe obstacles to understanding, but only in the sense that the task is harder than might naively have been supposed, not that it is impossible.

Consider, for instance, molecular biology. Some people once thought, perhaps, that one could examine a part of a molecule and discern that it was a gene for blue eyes, or homosexuality, or whatever, meaning that somehow encoded in the molecule was its tendency to bring about this trait in the organism of which it was part. Many journalists still seem to believe this. Now, however, no serious biologist thinks anything of the sort, if any ever did. A more sophisticated view is that a gene is a part of a molecule the function of which is to generate the production of a particular protein. But now it is understood that the pathway from gene to protein involves multiple steps: excision of parts of the sequence and their resplicing into a variety of new sequences; subsequent editing and packaging; folding into more or less specific structures, many of which collapse onto specific structures only in reaction with particular interactive partners, or substrates. A protein, in turn,

will often be capable of many different functions depending on its chemical context. The same molecule in a different kind of cell may serve a quite different set of functions. In sum, there is no reason to suppose there is any limited set of possible functions for a molecule, and to understand its biological significance it is essential to know a lot about the context to which it is related.

I am not, of course, suggesting that molecules obey rules, or have meanings. I am just noting that the holism, the necessity of knowing a lot about the context in which an object acts in order to understand its significance and functions, is characteristic of the natural, or at least the biological, sciences as much as of the social sciences.

A more promising interpretation of the implications of these two levels of involvement of meaning can also be found in Winch. If social science is to be of any use to us, then whatever specialised technical language the expert may elect to use, this must eventually be translated back into the ordinary language we can all understand. We want to find out about our social world, and we understand that world in ordinary language. If we want to understand the workings of the cell or the behaviour of black holes, on the other hand, we expect to have to learn something of the language of the relevant science. The importance of the stronger requirement on social language is frequently seen in the misunderstandings perpetuated especially by politicians through the language of economics. It is easy for the naïve consumer of this discourse to suppose that a decline in inflation will mean that the prices they

pay will rise more slowly; but the technical term from economics has no such implication. Perhaps more importantly, it is seldom remarked that there is no objectively correct measure of inflation, and any particular measure selected will have a different degree of accordance with the experience of an individual with a particular pattern of expenditure. I suppose it could be argued that “inflation” has become a term of ordinary language; but if so it is a term no very precise meaning of which is widely understood.

This observation leads to another important characteristic of the social sciences, a rather different way in which the normative is unavoidably implicated in their practice, that it makes little sense to consider them to be value-free. The language that we use to describe society is often a “thick” language, a language in which central terms have both descriptive and evaluative content. Failure to recognise this point is striking when, for example, evolutionary psychologists offer us allegedly value-free theories of the origins of aggression or rape. If there is a value neutral concept of rape that can be applied to the study of animals, then it is not our ordinary language concept. Here is a more insidious implication of the necessity of translating the findings of social science into the language we use and understand. When we are told that ducks or flies commit acts of rape, and are encouraged to infer that this may well be in some sense a natural, evolved behaviour for humans, we are equivocating between a supposedly neutral observational sense of the term and our everyday normative sense.

How deep a divide does this show between the natural and the social sciences? I suggest that it shows a deep and familiar difference between the subject matters, but no obvious systematic difference between the epistemological standards that constitute these diverse investigations as scientific. If, as I suppose, our language for describing society and social institutions is profoundly impregnated with normative features, then it is unavoidable that we express its findings in this value-impregnated language. No such normative dimension applies to words such as “quark”, “quasar”, “enzyme”, or “clade”. Why? Because in general we don’t care about these things. Some people, no doubt, have deep epistemic concerns with such esoteric entities, but it is no surprise that science involves epistemic values.

#### **4. Social sciences as life sciences**

Let me return to Wittgenstein. Wittgenstein has given us a picture of language nicely envisioned as “a maze of little streets and squares, of old and new houses, and of houses with additions from various periods”. Our language is diverse, with countless different words serving countless different functions. These functions, moreover, are more or less open-ended. As we confront unfamiliar situations we constantly make decisions as to how to extend the use of a word, a thesis famously exhibited in many parts of Wittgenstein’s work, notoriously, perhaps, in the rule-following considerations. If one sees the houses in Wittgenstein’s allegorical city as words, then we might imagine their development over time, as a roof extension is added to make room for the children, the kitchen or bathroom is remodelled, an interior

wall moved her or there to meet changing needs for space. Language is not a fixed thing, but a process, responding adaptively to an ever-changing context.

It is striking that what I have just said about language echoes perfectly much of what we have come to understand about life, as well. Living systems are also dynamic, open-ended processes, which respond adaptively to a changing environment. This is most familiar for evolutionary processes, but it is increasingly well understood that just the same is true of developmental processes. Organisms do not follow mechanistic, deterministic pathways from zygote to death. Development depends on an intricate blend of influences both from the chemistry and geometry of the developing organism and from the environment in which it develops. This enables development to respond adaptively, or functionally, to its developmental environment. Such developmental plasticity can be discerned at all levels in living systems, down to the biochemical (West-Eberhard 2003). Here it will be more relevant to focus on plasticity at a grosser level.

There are many strategies for this developmental flexibility. Many plants show remarkable morphological plasticity, diverting their energy into stems, roots, leaves, flowers, and so on in response to multiple contingencies of the environment they encounter. Animals, though they also exhibit morphological plasticity, have tended rather to channel much of their adaptive plasticity into behaviour. As they develop animals will acquire a range of behavioural capacities, and the set of such capacities will depend to some extent on their environment. These may take the form of highly predictable responses to

stimuli, or capacities that can be exercised as a result of some more open decision-making process. It is hardly controversial that humans have taken both the diversity of potential capacities, and the complexity of decision-making processes to an extent unmatched in the animal kingdom. The diversity of capacities, in particular, is what makes possible the complexity of human societies, grounded in an intricate division of labour.

The point of these fairly banal observations is to emphasise the continuity not only of the general approach of the social sciences with the biological sciences (and a fortiori the natural sciences), but also their subject matter. Equally I would stress the similarities between the methods of the social and natural sciences. This is not to say that they are very similar, still less to claim, whatever this means, that we are *just* animals, and there are no special problems for the social sciences. On the contrary, as I have argued for many years, the sciences are extremely diverse. There are many similarities and differences. Structural molecular biology is very different from taxonomy, and both are very different from high energy physics. Population ecology uses methods much more similar to economics than to astronomy or physiology. And so on. It is not that they are very similar, but that there is no obvious chasm between the natural and social sciences. We extend our concept of science, to adapt a useful analogy, “as in spinning a thread we twist fibre on fibre” (Wittgenstein 1953, §67).

I have said that the findings of the social sciences must, to be useful, be expressible in the ordinary language that we all understand. Why is this not

the basis of a conceptual divide between the natural and social sciences?

The answer is a point perhaps more important than the multiple family resemblances between the sciences. The language of science is not so different from any other part of language. Perhaps excessive attention to the peculiarities of mathematics has obscured this point, and given the impression that the language of science is uniquely precise. In fact both scientific and non-scientific words have both precise and not so precise meanings. In ordinary language, alongside such a paradigmatic family resemblance concept as “tool” we have perfectly exact terms such as “oil filter wrench” or “picture frame clamp”. Science has very precise terms too, of course, but the most important terms are far from exact. As I explained earlier, such terms as “species” and “gene” are far from exact terms, while terms for particular genes or species such as “sonic hedgehog” or “Lesser Spotted Dogfish” may be quite precise. Currently biologists debate whether bacteria or viruses form species, or whether promoters are genes. These are not so much debates about the nature of phenomena as about how to extend the use of a highly flexible term. Hans-Joerg Rheinberger (1997) has persuasively argued that this flexibility is vital for the progress of science. At any rate, we should recall Wittgenstein’s reminder that “inexact” does not mean “unusable” (Wittgenstein 1953, §88).

My conclusion, then, is that Wittgenstein’s profound insights about language apply very much the same way to the language of science as to the more mundane parts of our linguistic equipment. Science, social or otherwise, should be as much a part of the remit of his philosophical insights into the



workings of language as any other part of human life. Most of science should also be seen as “a maze of little streets and squares, of old and new houses, and of houses with additions from various periods”. If there are “new boroughs with straight regular streets and uniform houses”, there are surely not a multitude of these. Wittgenstein picks as examples of the latter chemical symbolism and calculus, which are certainly reasonable candidates—though anyone attempting to understand the complexities of protein chemistry, so-called intrinsically disordered proteins, for example, which exist in a suite of different conformations among which they rapidly pass until temporarily stabilised by interaction with a suitable substrate, might doubt how far the architectural regularity extended into the chemical suburb.

More general conceptions of science should stress rather a set of epistemic virtues, such as sensitivity to empirical fact, openness to critical engagement from diverse sources, coherence with other well-established beliefs, perhaps aesthetic virtues such as elegance and simplicity, and so on. It is the absence or violation of such virtues that licenses application of the trouser word, “unscientific”. The diversity of such values, moreover, makes science an obvious candidate for interpretation in the light of the idea of family resemblance: not all sciences possess all such virtues, or to the same degree, and this is entirely appropriate in view of the diversity of subject matter. In addition to the diversity of subject matter, recent philosophy of science has stressed the importance of multiple perspectives on the same subject matter, another space for divergent methods and virtues. One reason it is important to highlight this diversity of scientific methods and practices is that attempts to

ape the imagined systematic methods of the natural sciences has led the social sciences down many blind alleys. There are no such systematic methods, just various more or less successful methods for investigating a huge range of phenomena; social science should be free to develop its own. Perhaps it has so far been less successful than many of the natural sciences; but if so it is because it is harder, or perhaps it has been less well-done, not because it has failed to recognise some mythical scientific method.

I don't know whether Wittgenstein had a confused view of science of the kind I have gestured towards. He seems to have said very little about social science. If he had such a view, it is very understandable given the views of the time, dominated by the model of physics and, in philosophy, by the ambitions of logical positivism and logical empiricism. Now at any rate, we have moved beyond these views of science, and extended philosophical concerns far beyond physics. We can watch the construction of the winding streets and charming piazzas of the social sciences with philosophical equanimity.

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